ON THE MAGNITUDE OF THE SOLAR SYSTEM.[1]

NATURE may be studied in two widely different ways. On the one hand, we may employ a powerful microscope which will render visible the minutest forms and limit our field of view to an infinitesimal fraction of an inch situated within a foot of our own noses; or, on the other hand, we may occupy some commanding position, and from thence, aided perhaps by a telescope, we may obtain a comprehensive view of an extensive region. The first method is that of the specialist, the second is that of the philosopher, but both are necessary for an adequate understanding of nature. The one has brought us knowledge wherewith to defend ourselves against bacteria and microbes which are among the mostly deadly enemies of mankind, and the other has made us acquainted with the great laws of matter and force upon which rests the whole fabric of science. All nature is one, but for convenience of classification we have divided our knowledge into a number of sciences which we usually regard as quite distinct from each other. Along certain lines, or, more properly, in certain regions, these sciences necessarily abut on each other, and just there lies the weakness of the specialist. He is like a wayfarer who always finds obstacles in crossing the boundaries between two countries, while to the traveller who gazes over them from a commanding eminence the case is quite different. If the boundary is an ocean shore, there is no mistaking it; if a broad river or a chain of mountains, it is still distinct; but if only a line of posts traced over hill and dale, then it becomes lost in the natural features of the landscape, and the essential unity of the whole region is apparent. In that case the border-land is wholly a human conception of which nature takes no cognisance, and so it is with the scientific border-land to which I propose to invite your attention this evening.

To the popular mind there are no two sciences further apart than astronomy and geology. The one treats of the structure and mineral constitution of our earth, the causes of its physical features and its history; while the other treats of the celestial bodies, their magnitudes, motions, distances, periods of revolution, eclipses, order, and of the causes of their various phenomena. And yet many, perhaps I may even say most, of the apparent motions of the heavenly bodies are merely reflections of the motions of the earth, and in studying them we are really studying it. Furthermore, precision, mutation, and the phenomena of the tides depend largely upon the internal structure of the earth, and there astronomy and geology merge into each other. Nevertheless the methods of the two sciences are widely different, most astronomical problems being discussed quantitatively by means of rigid mathematical formulæ, while in the vast majority of cases the geological ones are discussed only qualitatively, each author contenting himself with a mere statement of what he thinks. With precise data the methods of astronomy lead to very exact results, for mathematics is a mill which grinds exceeding fine; but after all, what comes out of a mill depends wholly upon what is put into it, and if the data are uncertain, as is the case in most cosmological problems, there is little to choose between the mathematics of the astronomer and the guesses of the geologist.

If we examine the addresses delivered by former presidents of this Association, and of the sister—perhaps it would be nearer the truth to say the parent Association on the other side of the Atlantic—we shall find that they have generally dealt either with the recent advances in some broad field of science, or else with the development of some special subject. This evening I propose to adopt the latter course, and I shall invite your attention to the present condition of our knowledge respecting the magnitude of the solar system; but in so doing, it will be necessary to introduce some considerations derived from laboratory experiments upon the luminiferous ether, others derived from experiments from ponderable matter, and still others relating both to the surface phenomena and to the internal structure of the earth, and thus we shall deal largely with the border-land where astronomy, physics, and geology merge into each other.

The relative distances of the various bodies which compose the solar system can be determined to a considerable degree of approximation with very crude instruments as soon as the true plan of the system becomes known, and that plan was taught by Pythagoras more than five hundred years before Christ. It must have been known to the Egyptians and Chaldeans still earlier, if Pythagoras really acquired his knowledge of astronomy from them, as is affirmed by some of the ancient writers, but on that point there is no certainty. In public Pythagoras seemingly accepted the current belief of his time, which made the earth the centre of the universe, but to his own chosen disciples he communicated the true doctrine that the sun occupies the centre of the solar system, and that the earth is only one of the planets revolving around it. Like all the world's greatest sages, he seems to have taught only orally. A century elapsed before his doctrines were reduced to writing by Philolaus of Crotona, and it was still later before they were taught in public for the first time by Hicetas, or, as he is sometimes called, Nicetas, of Syracuse. Then the familiar cry of impiety was raised, and the Pythagorean system was eventually suppressed by that now called the Ptolemaic, which held the field until it was overthrown by Copernicus almost two thousand years later. Pliny tells us that Pythagoras believed the distances to the sun and moon to be respectively 252,000 and 12,600 stadia, or taking the stadium at 625 feet, 29,837 and 1492 English miles; but there is no record of the method by which these numbers were ascertained.

After the relative distances of the various planets are known, it only remains to determine the scale of the system, for which purpose the distance between any two planets suffices. We know little about the early history of the subject, but it is clear that the primitive astronomers must have found the quantities to be measured too small for detection with their instruments, and even in modern times the problem has proved to be an extremely difficult one. Aristarcus of Samos, who flourished about 270 B.C., seems to have been the first to attack it in a scientific manner.

Stated in modern language, his reasoning was that when the moon is exactly half full, the earth and sun as seen from its centre must make a right angle with each other, and by measuring the angle between the sun and moon, as seen from the earth at that instant, all the angles of the triangle joining the earth, sun, and moon would become known, and thus the ratio of the distance of the sun to the distance of the moon would be determined. Although perfectly correct in theory, the difficulty of deciding visually upon the exact instant when the moon is half full is so great that it cannot be accurately done even with the most powerful telescopes. Of course, Aristarcus had no telescope, and he does not explain how he effected the observation, but his conclusion was that at the instant in question the distance between the centres of the sun and moon, as seen from the earth, is less than a right angle by 1/30 part of the same. We should now express this by saying that the angle is 87°; but Aristarcus knew nothing of trigonometry, and in order to solve his triangle he had recourse to an ingenious but long and cumbersome geometrical process which has come down to us, and affords conclusive proof of the condition of Greek mathematics at that time. His conclusion was that the sun is nineteen times further from the earth than the moon, and if we combine that result with the modern value of the moon's parallax, viz. 3422·38 seconds, we obtain for the solar parallax 180 seconds, which is more than twenty times too great.

The only other method of determining the solar parallax known to the ancients was that devised by Hipparchus about 150 B.C. It was based on measuring the rate of decrease of the diameter of the earth's shadow cone by noting the duration of lunar eclipses, and as the result deduced from it happened to be nearly the same as that found by Aristarcus, substantially his value of the parallax remained in vogue for nearly two thousand years, and the discovery of the telescope was required to reveal its erroneous character. Doubtless this persistency was due to the extreme minuteness of the true parallax, which we now know is far too small to have been visible upon the ancient instruments, and thus the supposed measures of it were really nothing but measures of their inaccuracy.

The telescope was first pointed to the heavens by Galileo in 1609, but it needed a micrometer to convert it into an accurate measuring instrument, and that did not come into being until 1639, when it was invented by William Gascoigne. After his death in 1644, his original instrument passed to Richard Townley, who attached it to a fourteen-foot telescope at his residence in Townley, Lancashire, England, where it was used by Flamsteed in observing the diurnal parallax of Mars during its opposition in 1672. A description of Gascoigne's micrometer was published in the "Philosophical Transactions" in 1667, and a little before that a similar instrument had been invented by Auzout in France; but observatories were fewer then than now, and so far as I know J. D. Cassini was the only person beside Flamsteed who attempted to determine the solar parallax from that opposition of Mars. Foreseeing the importance of the opportunity, he had Richer despatched to Cayenne some months previously, and when the opposition came he effected two determinations of the parallax; one being by the diurnal method, from his own observations in Paris, and the other by the meridian method, from observations in France by himself, Romer and Picard, combined with those of Richer at Cayenne. This was the transition from the ancient instruments with open sights to telescopes armed with micrometers, and the result must have been little short of stunning to the seventeenth century astronomers, for it caused the hoary and gigantic parallax of about 180 seconds to shrink incontinently to ten seconds, and thus expanded their conception of the solar system to something like its true dimensions. More than fifty years previously Kepler had argued from his ideas of the celestial harmonies that the solar parallax could not exceed sixty seconds, and a little later Horrocks had shown on more scientific grounds that it was probably as small as fourteen seconds, but the final death-blow to the ancient values, ranging as high as two or three minutes, came from these observations of Mars by Flamsteed, Cassini, and Richer.

---

[1] Address delivered before the American Association for the Advancement of Science, at its Brooklyn Meeting, August 16, by the retiring President, William Harkness.

Of course the results obtained in 1672 produced a keen desire on the part of astronomers for further evidence respecting the true value of the parallax, and as Mars comes into a favourable position for such investigations only at intervals of about sixteen years, they had recourse to observations of Mercury and Venus. In 1677 Halley observed the diurnal parallax of Mercury, and also a transit of that planet across the sun's disc at St. Helena, and in 1681 J. D. Cassini and Picard observed Venus when she was on the same parallel with the sun; but although the observations of Venus gave better results than those of Mercury, neither of them was conclusive, and we now know that such methods are inaccurate, even with the powerful instruments of the present day. Nevertheless, Halley's attempt by means of the transit of Mercury ultimately bore fruit in the shape of his celebrated paper of 1716, wherein he showed the peculiar advantages of transits of Venus for determining the solar parallax. The idea of utilising such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks; but Halley was the first to work it out completely, and long after his death his paper was mainly instrumental in inducing the Governments of Europe to undertake the observations of the transits of Venus in 1761 and 1769, from which our first accurate knowledge of the sun's distance was obtained.

Those who are not familiar with practical astronomy may wonder why the solar parallax can be got from Mars and Venus, and not from Mercury, or the sun itself. The explanation depends on two facts—firstly, the nearest approach of these bodies to the earth is for Mars 33,874,000 miles, for Venus 23,654,000 miles, for Mercury 47,935,000 miles, and for the sun 91,239,000 miles. Consequently, for us, Mars and Venus have very much larger parallaxes than Mercury or the sun, and of course the larger the parallax the easier it is to measure. Secondly, even the largest of these parallaxes must be determined within far less than one-tenth of a second of the truth; and while that degree of accuracy is possible in measuring short arcs, it is quite unattainable in long ones. Hence one of the most essential conditions for the successful measurement of parallaxes is that we shall be able to compare the place of the near body with that of a more distant one situated in the same region of the sky. In the case of Mars, that can always be done by making use of a neighbouring star, but when Venus is near the earth she is also so close to the sun that stars are not available, and consequently her parallax can be satisfactorily measured only when her position can be accurately referred to that of the sun; or, in other words, only during her transits across the sun's disk. But even when the two bodies to be compared are sufficiently near each other, we are still embarrassed by the fact that it is more difficult to measure the distance between the limb of a planet and a star or the limb of the sun, than it is to measure the distance between two stars; and since the discovery of so many asteroids, that circumstance has led to their use for determinations of the solar parallax. Some of these bodies approach within 75,230,000 miles of the earth's orbit, and as they look precisely like stars, the increased accuracy of pointing on them fully makes up for their greater distance, as compared with Mars or Venus.

After the Copernican system of the world and the Newtonian theory of gravitation were accepted, it soon became evident that trigonometrical measurements of the solar parallax might be supplemented by determinations based on the theory of gravitation, and the first attempts in that direction were made by Machin in 1729 and T. Mayer in 1753. The measurement of the velocity of light between points on the earth's surface, first effected by Fizeau in 1849, opened up still other possibilities, and thus for determining the solar parallax we now have at our command no less than three entirely distinct classes of methods, which are known respectively as the trigonometrical, the gravitational, and the photo-tachymetrical. We have already given a summary sketch of the trigonometrical methods, as applied by the ancient astronomers to the dichotomy and shadow cone of the moon, and by the moderns to Venus, Mars, and the asteroids, and we shall next glance briefly at the gravitational and photo-tachymetrical methods.

The gravitational results which enter directly or indirectly into the solar parallax are six in number, to wit: first, the relation of the moon's mass to the tides; second, the relation of the moon's mass and parallax to the force of gravity at the earth's surface; third, the relation of the solar parallax to the masses of the earth and moon; fourth, the relation of the solar and lunar parallaxes to the moon's mass and parallactic inequality; fifth, the relation of the solar and lunar parallaxes to the moon's mass and the earth's lunar inequality; sixth, the relation of the constants of nutation and precession to the moon's parallax.

Respecting the first of these relations, it is to be remarked that the tide-producing forces are the attraction of the sun and moon upon the waters of the ocean, and from the ratio of these attractions the moon's mass can readily be determined. But unfortunately the ratio of the solar tides to the lunar tides is affected both by the depth of the sea and by the character of the channels through which the water flows, and for that reason the observed ratio of these tides requires multiplication by a correcting factor in order to convert it into the ratio of the forces. The matter is further complicated by this correcting factor varying from port to port, and in order to get satisfactory results long series of observations are necessary. The labour of deriving the moon's mass in this way was formerly so great that for more than half a century La Place's determination from the tides at Brest remained unique, but the recent application of harmonic analysis to the data supplied by self-registering tide gauges is likely to yield abundant results in the near future.

Our second gravitational relation, viz. that connecting the moon's mass and parallax with the force of gravity at the earth's surface, affords an indirect method of determining the moon's parallax with very great accuracy if the computation is carefully made, and with a fair approximation to the truth even when the data are exceedingly crude. To illustrate this, let us see what could be done with a railroad transit such as is commonly used by surveyors, a steel tape, and a fairly good watch. Neglecting small corrections due to the flattening of the earth, the centrifugal force at its surface, the eccentricity of its orbit, and the mass of the moon, the law of gravitation shows that if we multiply together the length of the seconds pendulum, the square of the radius of the earth, and the square of the length of the sidereal month, divide the product by four, and take the cube root of the quotient, the result will be the distance from the earth to the moon. To find the length of the seconds pendulum we would rate the watch by means of the railroad transit, and then making a pendulum out of a spherical leaden bullet suspended by a fine thread, we would adjust the length of the thread until the pendulum made exactly 300 vibrations in five minutes by the watch. Then, supposing the experiment to be made here, or in New York city, we would find that the distance from the point of suspension of the thread to the centre of the bullet was about 39 and 1/8 inches, and dividing that by the number of inches in a mile, viz. 63,360, we would have for the length of the seconds pendulum one-sixteen hundred and twentieth of a mile. The next step would be to ascertain the radius of the earth, and the quickest way of doing so would probably be, first, to determine the latitude of some point in New York city by means of the railroad transit; next, to run a traverse survey along the old Post Road from New York to Albany, and finally, to determine the latitude of some point in Albany. The traverse survey should surely be correct to one part in three hundred, and as the distance between the two cities is about two degrees, the difference of latitude might be determined to about the same percentage of accuracy. In that way we would find the length of two degrees of latitude to be about 138 miles, whence the earth's radius would be 3953 miles. It would then only remain to observe the time occupied by the moon in making a sidereal revolution around the earth, or, in other words, the time which she occupies in moving from any given star back to the same star again. By noting that to within one-quarter of her own diameter we should soon find that the time of a revolution is about 27·32 days, and multiplying that by the number of seconds in a day, viz. 86,400, we would have for the length of the sidereal month 2,360,000 seconds. With these data the computation would stand as follows:—The radius of the earth, 3953 miles, multiplied by the length of a sidereal month, 2,360,000 seconds, and the product squared, gives 87,060,000,000,000,000,000. Multiplying that by one-fourth of the length of the seconds pendulum, viz. 1/6480 of a mile, and extracting the cube root of the product, we would get 237,700 miles for the distance from the earth to the moon, which is only about 850 miles less than the truth, and certainly a remarkable result considering the crudeness of the instruments by which it might be obtained. Nevertheless, when all the conditions are rigorously taken into account, these data are to be regarded as determining the relation between the moon's mass and parallax rather than the parallax itself.

Our third gravitational relation, to wit, that existing between the solar parallax, the solar attractive force and the masses of the earth and moon, is analogous to the relation existing between the moon's mass and parallax and the force of gravity at the earth's surface, but it cannot be applied in exactly the same way, on account of our inability to swing a pendulum on the sun. We are therefore compelled to adopt some other method of determining the sun's attractive force, and the most available is that which consists in observing the perturbative action of the earth and moon upon our nearest planetary neighbours, Venus and Mars. From this action the law of gravitation enables us to determine the ratio of the sun's mass to the combined masses of the earth and moon, and then the relation in question furnishes a means of comparing the masses so found with trigonometrical determinations of the solar parallax. Thus it appears that notwithstanding necessary differences in the methods of procedure, the analogy between the second and third gravitational relations holds not only with respect to their theoretical basis, but also in their practical application, the one being used to determine the relation between the mass of the moon and its distance from the earth, and the other to determine the relation between the combined masses of the earth and noon and their distance from the sun.

Our fourth gravitational relation deals with the connection between the solar parallax, the lunar parallax, the moon's mass and the moon's parallactic inequality. The important quantities are here the solar parallax and the moon's parallactic inequality, and although the derivation of the complete expression for the connection between them is a little complicated, there is no difficulty in getting a general notion of the forces involved. As the moon moves around the earth she is alternately without and within the earth's orbit. When she is without, the sun's attraction on her acts with that of the earth; when she is within, the two attractions act in opposite directions. Thus in effect the centrifugal force holding the moon to the earth is alternately increased and diminished, with the result of elongating the moon's orbit towards the sun and compressing it on the opposite side. As the variation of the centrifugal force is not great, the change of the form of the orbit is small, nevertheless the summation of the minute alterations thereby produced in the moon's orbital velocity suffices to put her sometimes ahead, and sometimes behind her mean place to an extent which oscillates from a maximum to a minimum as the earth passes from perihelion to aphelion, and averages about 125 seconds of arc. This perturbation of the moon's node is known as the parallactic inequality because it depends on the earth's distance from the sun, and can therefore be expressed in terms of the solar parallax. Conversely, the solar parallax can be deduced from the observed value of the parallactic inequality, but unfortunately there are great practical difficulties in making the requisite observations with a sufficient degree of accuracy. Notwithstanding the ever-recurring talk about the advantages to be obtained by observing a small well-defined crater instead of the moon's limb, astronomers have hitherto found it impracticable to use anything but the limb, and the disadvantage of doing so as compared with observing a star is still further increased by the circumstances that in general only one limb can be seen at a time, the other being shrouded in darkness. If both limbs could always be observed, we should then have a uniform system of data for determining the place of the centre, but under existing circumstances we are compelled to make our observations half upon one limb and half upon the other, and thus they involve all the systematic errors which may arise from the conditions under which these limbs are observed, and all the uncertainty which attaches to irradiation, personal equation, and our defective knowledge of the moon's semidiameter.

Our fifth gravitational relation is that which exists between the solar parallax, the lunar parallax, the moon's mass, and the earth's lunar inequality. Strictly speaking, the moon does not revolve around the earth's centre, but both bodies revolve around the common centre of gravity of the two. In consequence of that an irregularity arises in the earth's orbital velocity around the sun, the common centre of gravity moving in accordance with the laws of elliptic motion, while the earth, on account of its revolution around that centre, undergoes an alternate acceleration and retardation which has for its period a lunar month, and is called the lunar inequality of the earth's motion. We perceive this inequality as an oscillation superposed on the elliptic motion of the sun, and its semi-amplitude is a measure of the angle subtended at the sun by the interval between the centre of the earth and the common centre of gravity of the earth and moon. Just as an astronomer on the moon might use the radius of her orbit around the earth as a base for measuring her distance from the sun, so we may use this interval for the same purpose. We find its length in miles from the equatorial semidiameter of the earth, the moon's parallax and the moon's mass, and thus we have all the data for determining the solar parallax from the inequality in question. In view of the great difficulty which has been experienced in measuring the solar parallax itself, it may be asked why we should attempt to deal with the parallactic inequality which is about twenty-six per cent. smaller? The answer is, because the latter is derived from differences of the sun's right ascension which are furnished by the principal observatories in vast numbers, and should give very accurate results on account of their being made by methods which insure freedom from constant errors. Nevertheless, the sun is not so well adapted for precise observations as the stars, and Dr. Gill has recently found that heliometer measurements upon asteroids which approach very near to the earth yield values of the parallactic inequality superior to those obtained from right ascensions of the sun.

Our sixth gravitational relation is that which exists between the moon's parallax and the constants of precession and nutation. Every particle of the earth is attracted both by the sun and by the moon, but in consequence of the polar flattening the resultant of these attractions passes a little to one side of the earth's centre of gravity. Thus a couple is set up, which, by its action upon the rotating earth, causes the axis thereof to describe a surface which may be called a fluted cone, with its apex at the earth's centre. A top spinning with its axis inclined describes a similar cone, except that the flutings are absent, and the apex is at the point upon which the spinning occurs. For convenience of computation we resolve this action into two components, and we name that which produces the cone the luni-solar precession, and that which produces the flutings the nutation. In this phenomenon the part played by the sun is comparatively small, and by eliminating it we obtain a relation between the luni-solar precession, the nutation and the moon's parallax, which can be used to verify and correct the observed values of these quantities.

In the preceding paragraph we have seen that the relation between the quantities there considered depends largely upon the flattening of the earth, and thus we are led to inquire how and with what degree of accuracy that is determined. There are five methods, viz. one geodetic, one gravitational, and three astronomical. The geodetic method depends upon measurements of the length of a degree on various parts of the earth's surface, and with the data hitherto accumulated it has proved quite unsatisfactory. The gravitational method consists in determining the length of the seconds pendulum over as great a range of latitude as possible, and deducing therefrom the ratio of the earth's polar and equatorial semi-diameters by means of Clairaut's theorem. The pendulum experiments show that the earth's crust is less dense on mountain plateaux than at the sea coast, and thus for the first time we are brought into contact with geological considerations. The first astronomical method consists in observing the moon's parallax from various points on the earth's surface, and as these parallaxes are nothing else than the angular semi-diameter of the earth at the respective points as seen from the moon, they afford a direct measure of the flattening. The second and third astronomical methods are based upon certain perturbations of the moon which depend upon the figure of the earth, and should give extremely accurate results, but unfortunately very great difficulties oppose themselves to the exact measurement of the perturbations. There is also an astronomico-geological method which cannot yet be regarded as conclusive, on account of our lack of knowledge respecting the law of density which prevails in the interior of the earth. It is based upon the fact that a certain function of the earth's moments of inertia can be determined from the observed values of the coefficients of precession and nutation, and could also be determined from the figure and dimensions of the earth if we knew the exact distribution of matter in its interior. Our present knowledge on that subject is limited to a superficial layer not more than ten miles thick, but it is usual to assume that the deeper matter is distributed according to La Grange's law, and then by writing the function in question in a form which leaves the flattening indeterminate, and equating the expression so found to the value given by the precession and nutation, we readily obtain the flattening. As yet these six methods do not give consistent results, and so long as serious discrepancies remain between them, there can be no security that we have arrived at the truth.

It should be remarked that in order to compute the function of the earth's moments of inertia, which we have just been considering, we require not only the figure and dimensions of the earth and the law of distribution of density in its interior, but also its mean and surface densities. The experiments for determining the mean density have consisted in comparing the earth's attraction with the attraction either of a mountain, or of a known thickness of the earth's crust, or of a known mass of metal. In the case of mountains, the comparisons have been made with plumb-lines and pendulum; in the case of known layers of the earth's crust, they have been made by swinging pendulums at the surface and down in mines; and in the case of known masses, they have been made with torsion balances, fine chemical balances, and pendulums. The surface density results from a study of the materials composing the earth's crust; but notwithstanding the apparent simplicity of that process, it is doubtful if we have yet attained as accurate a result as in the case of the mean density.

Before quitting this part of our subject, it is important to point out that the luni-solar precession cannot be directly observed, but must be derived from the general precession. The former of these quantities depends only upon the action of the sun and moon, while the latter is affected in addition by the action of all the planets, and to ascertain what that is we must determine their masses. The methods of doing so fall into two great classes, according as the planets dealt with have or have not satellites. The most favourable case is that in which one or more satellites are present, because the mass of the primary follows immediately from their distances and revolution times, but even then there is a difficulty in the way of obtaining very exact results. By extending the observations over sufficiently long periods the revolution times can be ascertained with any desired degree of accuracy, but all measurements of the distance of a satellite from its primary are affected by personal equation, which we cannot be sure of completely eliminating, and thus a considerable margin of uncertainty is brought into the masses. In the cases of Mercury and Venus, which have no satellites, and to a certain extent in the case of the earth also, the only available way of ascertaining the masses is from the perturbations produced by the action of the various planets on each other. These perturbations are of two kinds, periodic and secular. When sufficient data have been accumulated for the exact determination of the secular perturbations, they will give the best results, but as yet it remains advantageous to employ the periodic perturbations also.

Passing now to the photo-tachymetrical methods, we have first to glance briefly at the mechanical appliances by which the tremendous velocity of light has been successfully measured. They are of the simplest possible character, and are based either upon a toothed-wheel, or upon a revolving mirror.

The toothed-wheel method was first used by Fizeau in 1849. To understand its operation, imagine a gun-barrel with a toothed-wheel revolving at right angles to its muzzle in such a way that the barrel is alternately closed and opened as the teeth and the spaces between them pass before it. Then, with the wheel in rapid motion, at the instant when a space is opposite the muzzle, let a ball be fired. It will pass out freely, and after traversing a certain distance, let it strike an elastic cushion and be reflected back upon its own path. When it reaches the wheel, if it hits a space it will return into the gun-barrel, but if it hits a tooth it will be stopped. Examining the matter a little more closely, we see that as the ball requires a certain time to go and return, if during that time the wheel moves through an odd multiple of the angle between a space and a tooth the ball will be stopped, while if it moves through an even multiple of that angle the ball will return into the barrel. Now imagine the gun-barrel, the ball, and the elastic cushion to be replaced respectively by a telescope, a light wave, and a mirror. Then if the wheel be moved at such a speed that the returning light wave struck against the tooth following the space through which it issued, to an eye looking into the telescope all would be darkness. If the wheel moved a little faster and the returning light wave passed through the space succeeding that through which it issued, the eye at the telescope would perceive a flash of light; and if the speed was continuously increased, a continual succession of eclipses and illuminations would follow each other according as the returning light was stopped against a tooth, or passed through a space further and further behind that through which it issued. Under these conditions the time occupied by the light in traversing the space from the wheel to the mirror and back again would evidently be the same as the time required by the wheel to revolve through the angle between the space through which the light issued and that through which it returned, and thus the velocity of light would become known from the distance between the telescope and the mirror together with the speed of the wheel. Of course the longer the distance traversed, and the greater the velocity of the wheel, the more accurate would be the result.

The revolving mirror method was first used by Foucault in 1862. Conceive the toothed-wheel of Fizeau's apparatus to be replaced by a mirror attached to a vertical axis, and capable of being put into rapid rotation. Then it will be possible so to arrange the apparatus that light issuing from the telescope shall strike the movable mirror and be reflected to the distant mirror, whence it will be returned to the movable mirror again, and being thrown back into the telescope will appear as a star in the centre of the field of view. That adjustment being made, if the mirror were caused to revolve at a speed of some hundred turns per second, it would move through an appreciable angle while the light was passing from it to the distant mirror and back again, and in accordance with the laws of reflection, the star in the field of the telescope would move from the centre by twice the angle through which the mirror had turned. Thus the deviation of the star from the centre of the field would measure the angle through which the mirror turned during the time occupied by light in passing twice over the interval between the fixed and revolving mirrors, and from the magnitude of that angle together with the known speed of the mirror, the velocity of the light could be calculated.

In applying either of these methods the resulting velocity is that of light when traversing the earth's atmosphere, but what we want is its velocity in space which we suppose to be destitute of ponderable material, and in order to obtain that the velocity in the atmosphere must be multiplied by the refractive index of air. The corrected velocity so obtained can then be used to find the solar parallax, either from the time required by light to traverse the semi-diameter of the earth's orbit, or from the ratio of the velocity of light to the orbital velocity of the earth.

Any periodic correction which occurs in computing the place of a heavenly body, or the time of a celestial phenomena, is called by astronomers an equation, and as the time required by light to traverse the semi-diameter of the earth's orbit first presented itself in the guise of a correction to the computed times of the eclipses of Jupiter's satellites, it has received the name of the light equation. The earth's orbit being interior to that of Jupiter, and both having the sun for their centre, it is evident that the distance between the two planets must vary from the sun to the difference of the radii of their respective orbits, and the time required by light to travel from one planet to the other must vary proportionately. Consequently, if the observed times of the eclipses of Jupiter's satellites are compared with the times computed upon the assumption that the two planets are always separated by their mean distance, it will be found that the eclipses occur too early when the earth is at less than its mean distance from Jupiter, and too late when it is further off, and from large numbers of such observations the value of the light equation has been deduced.

The combination of the motion of light through our atmosphere with the orbital motion of the earth gives rise to the annual aberration, all the phases of which are computed from its maximum value, commonly called the constant of aberration. There is also a diurnal aberration due to the rotation of the earth on its axis, but that is quite small, and does not concern us this evening. When aberration was discovered the corpuscular theory of light was in vogue, and it offered a charmingly simple explanation of the whole phenomenon. The hypothetical light corpuscles impinging upon the earth were thought to behave precisely like the drops in a shower of rain, and you all know that their apparent direction is affected by any motion on the part of the observer. In a calm day, when the drops are falling perpendicularly, a man standing still holds his umbrella directly over his head, but as soon as he begins to move forward he inclines his umbrella in the same direction, and the more rapidly he moves the greater must be its inclination in order to meet the descending shower. Similarly the apparent direction of oncoming light corpuscles would be affected by the orbital motion of the earth, so that in effect it would always be the resultant arising from combining the motion of the light with a motion equal and opposite to that of the earth. But since the falsity of the corpuscular theory has been proved that explanation is no longer tenable, and as yet we have not been able to replace it with anything equally satisfactory based on the now universally accepted undulatory theory. In accordance with the latter theory we must conceive the earth as ploughing its way through the ether, and the point which has hitherto baffled us is whether or not in so doing it produces any disturbance of the ether which affects the aberration. In our present ignorance on that point we can only say that the aberration constant is certainly very nearly equal to the ratio of the earth's orbital velocity to the velocity of light, but we cannot affirm that it is rigorously so.

The luminiferous ether was invented to account for the phenomena of light, and for two hundred years it was not suspected to have any other function. The emission theory postulated only the corpuscles which constitute light itself, but the undulatory theory fills all space with an imponderable substance possessing properties even more remarkable than those of ordinary matter, and to some of the acutest intellects the magnitude of this idea has proved an almost insuperable objection against the whole theory. So late as 1862 Sir David Brewster, who had gained a world-wide reputation by his optical researches, expressed himself as staggered by the notion of filling all space with some substance merely to enable a little twinkling star to send its light to us; but not long after Clerk Maxwell removed that difficulty by a discovery coextensive with the undulatory theory itself. Since 1845, when Faraday first performed his celebrated experiment of magnetising a ray of light, the idea that electricity is a phenomenon of the ether had been steadily growing, until at last Maxwell perceived that if such were the fact the rate of propagation of an electromagnetic wave must be the same as the velocity of light. At that time no one knew how to generate such waves, but Maxwell's theory showed him that their velocity must be equal to the number of electric units of quantity in the electromagnetic unit, and careful experiments soon proved that that is the velocity of light. Thus it was put almost beyond the possibility of doubt that the ether gives rise to the phenomena of electricity and magnetism, as well as to those of light, and perhaps it may even be concerned in the production of gravitation itself. What could be apparently more remote than these electric quantities and the solar parallax? And yet we have here a relation between them, but we make no use of it, because as yet the same relation can be far more accurately determined from experiments upon the velocity of light.

Now let us recall the quantities and methods of observation which we have found to be involved either directly or indirectly with the solar parallax. They are the solar parallax, obtained from transits of Venus, oppositions of Mars, and oppositions of certain asteroids; the lunar parallax, found both directly, and from measurements of the force of gravity at the earth's surface; the constants of precision, nutation, and aberration, obtained from observations of the stars; the parallactic inequality of the moon; the lunar inequality of the earth, usually obtained from observations of the sun, but recently found from heliometer observations of certain asteroids; the mass of the earth, found from the solar parallax, and also from the periodic and secular perturbations of Venus and Mars; the mass of the moon, found from the lunar inequality of the earth, and also from the ratio of the solar and lunar components of the ocean tides; the masses of all the planets, obtained from observations of their satellites whenever possible, and when no satellites exist, then from observations of their mutual perturbations both periodic and secular; the velocity of light, obtained from experiments with revolving mirrors and toothed wheels, together with laboratory determinations of the index of refraction of atmospheric air; the light equation, obtained from observations of the ellipses of Jupiter's satellites; the figure of the earth, obtained from geodetic triangulations, measurements of the length of the seconds pendulum in various latitudes, and observations of certain perturbations of the moon; the mean density of the earth, obtained from measurements of the attractions of mountains, from pendulum experiments in mines, and from experiments on the attraction of known masses of matter made either with torsion balances or with the most delicate chemical balances; the surface density of the earth, obtained from geological examinations of the surface strata; and lastly, the law of distribution of density in the interior of the earth, which in the present state of geological knowledge we can do little more than guess at.

Here then we have a large group of astronomical, geodetic, geological and physical quantities which must all be considered in finding the solar parallax, and which are all so entangled with each other that no one of them can be varied without affecting all the rest. It is therefore impossible to make an accurate determination of any one of them apart from the remainder of the group, and thus we are driven to the conclusion that they must all be determined simultaneously. Such has not been the practice of astronomers in the past, but it is the method to which they must inevitably resort in the future. A cursory glance at an analogous problem occurring in geodesy may be instructive. When a country is covered with a net of triangles it is always found that the observed angles are subject to a certain amount of error, and a century ago it was the habit to correct the angles in each triangle without much regard to the effect upon adjacent triangles. Consequently the adjustment of the errors was imperfect, and in computing the interval between any two distant points the result would vary somewhat with the triangles used in the computation—that is, if one computation was made through a chain of triangles running around on the right-hand side, another through a chain of triangles running straight between the two points, and a third through a chain of triangles running around on the left-hand side, the results would usually all differ. At that time things were less highly specialised than now, and all geodetic operations were yet in the hands of first-rate astronomers who soon devised processes for overcoming the difficulty. They imagined every observed angle to be subject to a small correction, and as these corrections were all entangled with each other through the geometrical conditions of the net, by a most ingenious application of the method of least squares they determined then all simultaneously in such a way as to satisfy the whole of the geometrical conditions. Thus the best possible adjustment was obtained, and no matter what triangles were used in passing from one point to another, the result was always the same. That method is now applied to every important triangulation, and its omission would be regarded as proof of incompetency on the part of those in charge of the work.

Now let us compare the conditions existing respectively in a triangulation net and in the group of quantities for the determination of the solar parallax. In the net every angle is subject to a small correction, and the whole system of corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy all the geometrical conditions of the net. Like the triangles, the quantities composing the group from which the solar parallax must be determined are all subject to error, and therefore we must regard each of them as requiring a small correction, and all these corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy every one of the equations expressing the relations between the various components of the group.

Thus it appears that the method required for adjusting the solar parallax and its related constants is in all respects the same as that which has so long been used for adjusting systems of triangulation; and as the latter method was invented by astronomers, it is natural to inquire why they have not applied it to the fundamental problem of their own science? The reasons are various, but they may all be classed under two heads. First, an inveterate habit of over-estimating the accuracy of our own work as compared with that of others; and second, the unfortunate effect of too much specialisation.

The prevailing opinion certainly is that great advances have recently been made in astronomy, and so they have in the fields of spectrum analysis and in the measurements of minute quantities of radiant heat; but the solution of the vast majority of astronomical problems depends upon the exact measurement of angles, and in that little or no progress has been made. Bradley, with his zenith sector, a hundred and fifty years ago, and Bessel and Struve, with their circles and transit instruments, seventy years ago, made observations not sensibly inferior to those of the present day, and indeed it would have been surprising if they had not done so. The essentials for accurately determining star places are a skilled observer, a clock, and a transit circle, the latter consisting of a telescope, a divided circle, and four micrometer microscopes. Surely no one will claim that we have to-day any more skilful observers than were Bessel, Bradley, and Struve, and the only way in which we have improved upon the telescopes made by Dollond one hundred and thirty years ago, is by increasing their aperture and relatively diminishing their focal distance. The most famous dividing engine now in existence was made by the elder Repsold seventy-five years ago; but as the errors of divided circles and their micrometer microscopes are always carefully determined, the accuracy of the measured angles is quite independent of any small improvement in the accuracy of the division or of the micrometer screws. Only in the matter of clocks has there been some advance, and even that is not very great. On the whole, the star places of to-day are a little better than those of seventy-five years ago, but even yet there is great room for improvement. One of the commonest applications of these star places is to the determination of latitude, but it is very doubtful if there is any point on the face of the earth whose latitude is known certainly within one-tenth of a second.

Looking at the question from another point of view, it is notorious that the contact observations of the transits of Venus in 1761 and 1769 were so discordant that from the same observations Encke and E. J. Stone got respectively for the solar parallax 8·59 seconds and 8·91 seconds. In 1870 no one thought it possible that there could be any such difficulty with the contact observations of then approaching transits of 1874 and 1882, but now we have found from sad experience that our vaunted modern instruments gave very little better results for the last pair of transits than our predecessors obtained with much cruder appliances in 1761 and 1769.

The theory of probability and uniform experience alike show that the limit of accuracy attainable with any instrument is soon reached; and yet we all know the fascination which continually lures us on in our efforts to get better results out of the familiar telescopes and circles which have constituted the standard equipment of observatories for nearly a century. Possibly these instruments may be capable of indicating somewhat smaller quantities than we have hitherto succeeded in measuring with them; but their limit cannot be far off, because they already show the disturbing effects of slight inequalities of temperature and other uncontrollable causes. So far as these effects are accidental, they eliminate themselves from every long series of observations, but there always remains a residuum of constant error, perhaps quite unsuspected, which gives us no end of trouble. Encke's value of the solar parallax affords a fine illustration of this. From the transits of Venus in 1761 and 1769 he found 8·58 seconds in 1824, which he subsequently corrected to 8·57 seconds, and for thirty years that value was universally accepted. The first objection to it came from Hansen in 1854, a second followed from Le Verrier in 1858, both based upon facts connected with the lunar theory, and eventually it became evident that Encke's parallax was about one quarter of a second too small. Now please observe that Encke's value was obtained trigonometrically, and its inaccuracy was never suspected until it was revealed by gravitational methods which were themselves in error about one-tenth of a second, and required subsequent correction in other ways. Here then was a lesson to astronomers, who are all more or less specialists, but it merely enforced the perfectly well-known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Le Verrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements, and do everything possible to clear up the mystery which now surrounds the constant of aberration; but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question, while he persistently neglected to adjust the net? And yet, until recently, astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How then can we suppose that the result properly deducible from them can be materially affected by anything that any of us can do in a lifetime, unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determination of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities, and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly 8·809 seconds ± 0·0057 seconds, whence we have for the mean distance between the earth and sun 92,797,000 miles, with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the Planet Neptune, 5,578,400,000 miles.

THE METEOR AND METEOR-STREAK OF AUGUST 26, 1894.

THE present year will certainly be remarkable for its large meteors. One of the most brilliant class of these phenomena appeared on January 25, and a fortnight later (February 8) a fireball was seen as a conspicuous object even in the presence of the midday sun, for the time was only 28 minutes after noon. The early evening of February 21 furnished another of these brilliant objects, but the observations were neither numerous nor exact, and all that could be definitely gleaned from them was that the body disappeared at a height of 30 miles over Bolton in Lancashire. On April 22, before daylight had gone, a fine meteor descended over the extreme south-east part of England, crossing the Strait of Dover from Hastings in the direction of Amiens in France. On May 18 a large daylight meteor was observed in Scotland and Ireland. Several additional instances of these striking visitors have been recently recorded, and the Perseids presented a few fine specimens, though the season has been a very cloudy and unpropitious one for all kinds of celestial observation.

The magnificent meteor which forms the subject of this paper, appeared on August 26 at 10h. 20m. It did not owe its parentage to the great Perseid system, for it came too late in the month, and, moreover, its direction of flight is not conformable.